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THE CORRESPONDENCE OF HUYGENS.

Œuvres complètes de Christiaan Huygens publiées par la Société Hollandaise des Sciences. Tome Huitième. Correspondance 1676-1684. Pp. 629. 4to. (La Haye: Nijhoff, 1899).

EXACTLY a year after the seventh volume of Huygens' correspondence the eighth one has made its appearance. As it embraces nine years, and Huygens only lived ten years longer, we may expect that the ninth volume will be the last one devoted to his correspondence. The objection which we made a year ago to the many comparatively uninteresting private letters which have contributed so much to the huge extent to which this collection has grown, applies equally to the present volume. No doubt Huygens was very glad to get the pleasant, gossiping letters from his sister Susanna, who kept him up to what was going on in Holland during his stay in Paris, but posterity will hardly be equally grateful to the far too conscientious editors who have considered it a duty to insert them among Huygens' "*Œuvres complètes*." One feels momentarily almost kindly disposed towards the niece of Peiresc, who ruthlessly destroyed ten thousand letters found after him.

Having suffered from ill-health for some months, Huygens left Paris for the Hague in the summer of 1676. In the autumn of the following year he wrote to Colbert to apologise for not coming back that winter owing to his health, but it appears from a letter to his brother Constantin that this was a mere excuse. To his absence from Paris at this particular time we owe some very interesting letters, exchanged between him and Roemer about the latter's discovery of the gradual propagation of light. Huygens had to return to Paris in the summer of 1678, as he did not wish to lose his French pension, but in 1681 he bade a final farewell to Paris and established himself in his native country. It has been generally supposed that he and Roemer (who had left the French capital a few months before him) were induced to do so by the feeling that Protestants were about to have a bad time in France, though as a matter of fact the edict of Nantes was not repealed till four years later. No doubt this feeling may have had something to do with their departure, but we learn from his correspondence that Huygens did not debar himself from returning, but kept a door open by writing from time to time to Colbert, and after his death to Louvois, regretting that the necessity of being near skilful workmen prevented him from returning yet, and expressing the hope that his pension might not be finally withdrawn. Doubtless he spoke his mind more honestly when he wrote to Constantin (September 1682) that he had no intention of living in France, partly on account of the three illnesses he had suffered from there, "and also for other reasons," but that he wanted to try to get some part of his pension without living in France. From the very last letter in the volume we see that he was in December 1684 still pegging away at Louvois, but the Minister of War of Louis XIV. no doubt considered that

money was too scarce to be spent on a foreign philosopher.

The skilful workmen, whom Huygens wanted in 1681 were required for the completion of his planetary machine, by which he claimed to represent the motions in the solar system with considerable accuracy. Huygens was very proud of the performance of this machine, which is still preserved at the Leiden Observatory, and in letters to Colbert and others he repeatedly lays stress on its superiority to the machine for the same purpose constructed by Roemer, which is also still in existence, in the "Round Tower" at Copenhagen, on the top of which the observatory was formerly situated. While working at this machine Huygens also wrote to Paris that he was engaged in the perfection of time-keepers for finding the longitude, and that he did so at the instance of the East India Company, but we hear nothing further about this matter. After his return to Holland he and Constantin resumed their investigations on the best methods of polishing lenses for telescopes, and in a number of letters they exchanged their ideas as to the proper construction of polishing machines, &c. In 1684 Christian Huygens published his "*Astroscopia compendiarum*," in which he described his method of using very long telescopes without tubes, keeping the eye-piece in the optical axis of the object-glass by means of a long string which connected two rods attached one to each. In a letter dated June 5, 1684, J. D. Cassini makes the very remarkable suggestion that the object-glass might be moved by a monster clock moving in the plane of the equator, on which for the hand was substituted a perpendicular plane to which the lens might be attached according to the declination of the star. We believe this to be the earliest suggestion of an equatorial moved by clockwork. On the other hand, Perrault, a month later, sent Huygens a design of a horizontal telescope into which the light from the star was thrown by a mirror kept in the proper position by an assistant, who pointed to the star with a small altazimuth tube connected with the mirror by a system of pulleys. He also sent a similar design by a certain Boffat. It is interesting to see that the horizontal telescope has been proposed so long ago.

During the period (1676-84) covered by the present volume several discoveries of the highest importance were given to the world, especially the discovery of the differential calculus by Leibnitz, but the communications exchanged between him and Huygens on this subject have already been printed in the collections of Uylensbroek and Gerhardt. Quite new, on the other hand, are the letters on Roemer's discovery of the gradual propagation of light, only the two first of which have been printed before (in Horrebow's "*Opera mathematico-physica*," T. iii. pp. 126-127, apparently not noticed by the editors). In reply to an inquiry from Huygens, who had seen in the *Phil. Trans.*, No. 136, a short account of Roemer's paper (laid before the Academy on November 22, 1676), Roemer informed him, under date September 30, 1677, that Picard acknowledged the reality of the discovery, but that Cassini did not, as only the first satellite of Jupiter showed the phenomenon. Roemer explained this by pointing out that the occultations of the outer satellites were less frequent, the moments of their occultations less sharply observable owing to the

slower motion and to their generally entering the shadow more obliquely; their inclinations and nodes were less accurately known, while it was well known that the motions of the outer satellites differed in a very irregular manner from Cassini's tables by amounts much larger than that dealt with in the case of the first satellite. The deviation in question was neither a function of the anomaly of Jupiter nor of that of the earth, nor of the configuration of the satellites, but solely of the distance from the earth. Writing to Colbert shortly afterwards, Huygens calls the discovery a most important one, in the confirmation of which the Royal Observatory would be worthily employed, and he adds that he was all the more pleased, as he had himself already, by means of this hypothesis, demonstrated the laws of the double refraction in Iceland spar. To Roemer he wrote that Cassini's objection did not trouble him much, as long as there were not better ephemerides of the outer satellites available. He doubted that observations of the surface-markings of Jupiter would be of any use in this inquiry, as they could not be accurate enough; but this Roemer did not acknowledge, since the time of passage of a spot across the central meridian could be fixed within two minutes. In a subsequent letter and in a communication to the Academy (which does not seem to have been printed before), Roemer proudly gives observations of a spot of September and December 1677, the comparison of which with an assumed value of the period of rotation seemed to exhibit the phenomenon beautifully. Of much greater interest is a remark made by Roemer in a letter dated December 30, 1677, in which he points out that the motion of the earth must affect the apparent direction of the path of light! In Cartesian language, he expresses this by saying that the circular motion of the terrestrial vortex must produce a curvature of the path, and he ingeniously suggests that the amount of this deflection might be determined by selecting two stars in the zodiac, nearly opposite each other, and observing their angular distance apart, first when one was at its heliacal rising, and again four or five months later when the other approached its heliacal setting. The difference would be four times the amount of the deflection, or, as we should say, four times the constant of aberration. It is very remarkable that Picard, Roemer's teacher and friend, should have discovered the changes in the place of the pole-star due to aberration (and also those due to nutation, though not the laws which regulate either—see his "Voyage d'Uranibourg," article viii.), while Roemer logically concluded from his discovery of the velocity of light that there ought to be aberration of light. But it was reserved for Bradley to publish both the laws and the theory of aberration. These facts become still more curious when we reflect that, but for the unfortunate destruction by fire of almost all Roemer's observations—which had been made with instruments constructed on novel principles not adopted elsewhere till much later, the foundation of modern astronomy might have been built on them and not on Bradley's observations. It was indeed unfortunate that Roemer published so very little about his scientific labours, and it is therefore particularly interesting to get a slight insight into them through his correspondence with Huygens.

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Among other matters dealt with in this volume we may mention the controversy on the theory of the centre of oscillation between Huygens and Abbé de Catelan, a man whose aim in life seems to have been to object to every new mathematical publication and to exhibit his inability to grasp any new theory. In Vol. i. of Huygens' "Opera varia," the papers written by the two opponents, as well as by Jacques Bernouilli, who took Huygens' part, have already been printed side by side; but it is interesting to see from the correspondence that Catelan's attack was slyly inserted in the Amsterdam reprint of the *Journal des Scavans*, although it had not appeared in the original Paris edition.

The volume contains as frontispiece a plate reproducing a fine medallion of Huygens from 1679, and another showing a medal apparently struck in his honour in the same year. It is announced that his unpublished works are to appear in the volumes following immediately after those devoted to his correspondence.

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METAPHYSICS OF BIOLOGY.

The Living Organism: an Introduction to the Problems of Biology. By Alfred Earl, M.A. Pp. xiii + 271. (London: Macmillan and Co., Ltd., 1898.)

THE observer of the more recent phases of biological thought will not need to be told that during the last few years a reaction has been setting in, both in England and abroad, against any so-called mechanical theories of the origin and development of living things, and against any hypothesis which seeks in the facts of chemistry and physics for an ultimate explanation of the phenomena of life; and those who have had the opportunity of a more intimate acquaintance with this new philosophical development will know that the "neo-vitalist" adopts, as the basis of his scientific beliefs, an ontology which states that it is not true that the hierarchy of the natural sciences presents us with a material universe of which the separate parts studied by the several sciences can all be ultimately expressed in terms of one of them, biology in fact being a special case of chemistry, this of physics and so on; but that on the contrary every science deals, not with a part, but with the whole of the material universe, all the facts of which come under its survey, and as a particular manner of looking at which it is to be regarded. On this view, therefore, it is as useless ever to expect a physical explanation of the chemical atom as it is futile to hope that organic metabolism may after all turn out to be merely a specially complex chemical reaction: each science has what is, for itself, an ultimate fact, in terms of which it seeks to express the whole of nature, but which has nothing in common with the ultimate fact of any other science whatever. This ultimate fact is, for the vitalistic biologist, the living organism, and when pressed for an account of how the inanimate world is included in his science, he replies by a reference to the environment, which, we are told, is to be regarded as being made by and for the organism itself.

Now it may be that the ontology which includes, with Kant, all phenomena in but a single category is obsolete,